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SOME PROBLEMS IN EVOLUTION[1]

IT was nearly 100 years ago that Charles Darwin began his scientific studies in the University of Edinburgh. In this illustrious center of intellectual activity he met various friends keenly interested in natural history, and attended the meetings of scientific societies, and it was doubtless here that were sown many of the seeds destined to bear such glorious fruit many years later. No more fitting subject, I think, could be found for an address than certain problems relating to his doctrine of evolution. That controversy perpetually rages round it is a healthy sign. For we must take care in science lest doctrine should pass into dogma, unquestioned and accepted merely on authority. So from time to time it is useful to reexamine in the light of new knowledge the very foundations on which our theories are laid.

Perhaps the best way of treating these general subjects is by trying to answer some definite questions. For instance, we may ask: "Why are some characters inherited and others not?" By characters we mean all those qualities and properties possessed by the organism, and by the enumeration of which we describe it; its weight, size, shape, color, its structure, composition and activities. Next, what do we mean by "inherited"? It is most important, if possible, clearly to define this term, since much of the controversy in writings on evolution is due to its use by various authors with a very different significance—sometimes as mere reappearance, at other times as actual transmission or transference from one generation to the next. Now, I propose to use the word inheritance merely to signify the reappearance in the offspring of a character possessed by the ancestor—a fact

[1] Address of the president of Section D—Zoology—British Association for the Advancement of Science, Edinburgh, September, 1921.

which may be observed and described, regardless of any theory as to its cause. Our question, then, is: "Why do some characters reappear in the offspring and others not?"

It is sometimes asserted that old-established characters are inherited, and that newly begotten ones are not, or are less constant, in their reappearance. This statement will not bear critical examination. For, on the one hand, it has been conclusively shown by experimental breeding that the newest characters may be inherited as constantly as the most ancient, provided they are possessed by both parents.[2] While, on the other hand, few characters in plants can be older than the green color due to chlorophyll, yet it is sufficient to cut off the light from a germinating seed for the greenness to fail to appear. Again, ever since Devonian times vertebrates have inherited paired eyes; yet, as Professor Stockard has shown, if a little magnesium chloride is added to the sea-water in which the eggs of the fish *Fundulus* are developing, they will give rise to embryos with one median cyclopean eye! Nor is the suggestion any happier that the, so to speak, more deep-seated and fundamental characters are more constantly inherited than the trivial or superficial. A glance at organisms around us, or the slightest experimental trial, soon convinces us that the apparently least-important character may reappear as constantly as the most fundamental. But while an organism may live without some trivial character, it can rarely do so when a fundamental character is absent, hence such incomplete individuals are seldom met in Nature.

Yet undoubtedly some characters reappear without fail and others do not. If it is neither age nor importance, what is it that determines their inheritance? The answer is that for a character to reappear in the offspring it is essential that the germinal factors and the environmental conditions which cooperated in its formation in the ancestor should both be present. Inheritance depends on this condition being fulfilled. For all characters are of the nature of responses to environment,[3] they are the products or results of the interaction between the factors of inheritance (germinal factors) and the surrounding conditions or stimuli. This power of response or reaction is no mysterious property of organisms—it is the effect produced, the disturbance brought about by the application of a stimulus. All the special properties and activities of living organisms ultimately depend on their metabolism, of which growth and reproduction are the chief manifestations. The course of metabolism, and, consequently, the development in the individual of a character, is molded or conditioned by the environmental stimuli under which it takes place. On the other hand, the living substance, protoplasm, which is undergoing metabolism, is the material basis of the organism. It has a specific composition and structure peculiar to the particular kind of organism concerned, and this is handed on to the offspring in the germ-cells from which starts the new generation. The inheritance of a character is due, then, not only to the actual transmission or transference of this specific "germ-plasm" containing the same factors of inheritance (germinal factors) as those from which the parent developed, but also to this factorial complex developing under the same conditions (environmental stimuli), as those under which the parent developed. Any alteration either in the effective environmental stimuli or in the germinal factors will produce a new result, will give rise to a new character, will cause the old character to appear no longer.

Now what is actually transmitted from one generation to the next is the complex of germinal factors. Hence we should carefully distinguish between transmission and inheritance.

[2] We purposely set aside complications due to hybridization and Mendelian segregation, which do not directly bear on the questions at issue.

[3] In a letter to *Nature* Sir Ray Lankester long ago drew attention to the importance of this consideration when discussing inheritance. He also pointed out that Lamarck's first law, that a new stimulus alters the characters of an organism, contradicts his second law, that the effects of previous stimuli are fixed by inheritance. (*Nature*, Vol. LI., 1894.)

Much of the endless confusion and interminable controversies about the inheritance of so-called "acquired characters" is due to the neglect of this important distinction. For it is quite clear that whereas factors may be transmitted, characters as such never are. The characters of the adult, being responses, are not present as such in the fertilized ovum from which it develops, they are produced anew at every generation.[4] No distinction in kind or value can be drawn between characters.

If some are inherited regularly and others are not, the distinction lies not in the nature or mode of production of the characters themselves, but in the constancy of the factors and conditions which give rise to them. Thus, although there is only one kind of character, there are two kinds of variation.

Much of the confusion in evolutionary literature is, I think, due to the use of the word variation in a loose manner. Sometimes it is taken to mean the degree of divergence between two individuals; sometimes the character itself in which they differ, such as a color or spot on a butterfly's wing, at other times a variety or race differing from the normal form of the species. If clearness of thought and expression is to be attained, the word variation should mean the extent or degree of difference between two individuals or between an individual and the average of the species, the divergence of the new form from the old; not a new character or assemblage of characters, but a difference which can be measured or at least estimated. We shall then find that a variation is of one of two kinds (which may, of course, be combined): the first kind is due to some change in the complex of germinal factors.

The second kind, to which the name mutation has been applied, will, under constant conditions, be inherited since the new complex of factors will be transmitted to subsequent generations. The first kind of variation, which has been called a modification, will also be inherited, provided, of course, the change of stimulus persists. In either case, new characters will result. But here, again, we must be careful not to apply the terms mutation and modification to the characters themselves, as is so often done;[5] for we then reintroduce the confusion already exposed in the popular but misleading distinction between "acquired" and "non-acquired" characters. The characters due to mutation or modification are, of course, indistinguishable by mere inspection, and can only be separated by experiment. A mutation once established should give rise, under uniform conditions, to a new heritable character, and may be detected by crossing with normal members of the species.

So far observations and tests have shown that new characters due to modification only reappear so long as the new stimulus persists. The difference lies not in the value or permanence of the new character, but in the causes which give rise to it.[6]

It is little more than a platitude to state that, for the production of an organism or of any of its characters, both germinal factors and environmental stimuli are necessary, and that if evolution is to take place there must be change in one or both. Yet the changes in the factors may be held to be the more important.

[4] In other words, all characters are "acquired during the lifetime of the individual," and "inherited" in the sense here defined has just the same meaning. Much the same view was advocated by Professor A. Sedgwick in his address to this Section at Dover in 1899, and it has also been developed by Dr. Archdall Reid and others.

[5] The name "mutation" might be given to the alteration in the factors instead of the variation due to it. The latter might then be termed a mutational variation and would be opposed to a modificational variation. At present the term "mutation" is applied to three different things: the factorial change, the variation or difference, and the new product response or character.

[6] We might perhaps distinguish the two cases by calling them constant and inconstant characters, or "natural" and "acquired," as is commonly done when describing immunity. It should be meant thereby that one is acquired usually (under normal conditions), the other occasionally (when infection occurs). Error creeps in when the term "acquired" is opposed to "non-acquired" or to "inherited."

In an environment which on the whole alters but little, evolution progresses by the cumulation along diverging lines of adaptation of new characters due to mutation. Thus natural selection indirectly preserves those factorial complexes which respond in a favorable manner. In other words, an organism, to survive in the struggle for existence, must present that assemblage of factors of inheritance which, under the existing environmental conditions, will give rise to advantageous characters.

In answer to a further question, let us now try to explain what we mean when we contrast the organism with its environment. In its simplest and most abstract form a living organism may be likened to a vortex. That mixture of highly complex proteins we call protoplasm, the physical basis of life, is perpetually undergoing transformations of matter and energy, so long as life persists. Towards the center of the vortex the highest compounds are continually being built up and continually being broken down; new material (food, water, oxygen) and energy are brought in at the periphery, and old material and energy (work and heat) thrown out. The principle of the conservation of energy and matter holds good in organized living processes as it does in the inorganic world outside. This is the process we call metabolism, and it is at the base of all the manifestations of life. From the point of view of biological science life is founded on a complex and continuous physico-chemical process of endless duration so long as conditions are favorable; just as a fire will continue to burn so long as fuel is at hand. No one step, no single substance, can be said to be living; the whole chain of substances and reactions, every link of which is essential, constitutes the life-process. A stream of non-living matter with stored-up energy is built up into the living vortex, and again passes out as dead matter, having yielded up the energy necessary for the performance of the various activities of the organism. If more is taken in than is given out it will grow and sub-divide. The complexity of the organism may increase by the formation of subsidiary, more or less interdependent, vortices within it. The perpetual growth and transmission of factors of inheritance, the continuity of the germ-plasm, is but another aspect of the continuity of the metabolic process forming the basis of the continuity of life in evolution.

But all the environmental stimuli are not external to the organism. Just as the various steps in the metabolic process are dependent on those which preceded them, so when an organism becomes differentiated into parts, when the main process becomes subdivided into subsidiary ones, these react on each other. What is internal to the whole becomes external to the part. An external stimulus may set up an internal metabolic change, giving rise to a response whose extent and nature depend on the structure of the mechanism and its state when stimulated, that is to say, on the effect of previous responses. Such a response may act as an internal stimulus giving rise to a further response, which may modify the first, and so on. Parts thus become marvelously fitted to set going, inhibit, or regulate each other's action; and thus arises that power of individual adaptation, or self-regulation, so characteristic of living organisms. The processes of temperature regulation, of respiration, of excretion are examples of such delicate self-regulating mechanisms in ourselves. But one of the great advantages thereby gained by organisms is that they can regulate their own growth and ensure their own "right" development. Whereas the simplest plants and animals are to a great extent, so to speak, at the mercy of their external environment, except in so far as they can move from unfavorable to more favorable surroundings; whereas their characters appear in response to external stimuli which may or may not be present, and over which they have little or no control—the higher organisms (more especially the higher animals), as it were, gradually substitute internal for external stimuli. Food material is provided in the ovum, and the size, structure and time of appearance of various characters are regulated to a great extent by use and by the secretions of various

endocrinal glands, the action of which has been so successfully studied, among others, by Sir Sharpey Schaffer in this University. Thus, as is well shown in man, the higher animals acquire considerable independence, and are little affected in their development by minor changes of environment. Inheritance is thus made secure by ensuring that the necessary conditions are always present.

We may seem to have wandered far from our original question; but the answer now appears to be that only those characters can be regularly inherited which depend for their appearance on conditions always fulfilled in the normal environment (external or internal); and those characters will not be regularly inherited which depend on stimuli that may or may not be present. Thus, while the offspring of a dark-skinned race will be dark in whatever climate they are born, those of a fair-skinned race will be born fair, but may be darkened by sun-burn, if they spend their holiday in the open.

Now it will be said, and not without some truth, that all this is mere commonplace admitted by all; but, if so, it is, I think, often ignored or misunderstood in discussions on heredity, more especially in semi-popular writings, and sometimes even in scientific works. However, I quite willingly admit that the real problems Darwin left to be solved by the evolutionist are the nature of the germinal factors themselves, and more especially the origin of the differences between them, the origin of those changes which give rise to mutations.

That these factors[7] must at least be self-propagating substances, subsidiary vortices in the main stream of metabolizing living protoplasm, is certain, since they grow and multiply repeatedly, to be distributed to new generations of germ-cells. That they may be relatively constant and remain unaltered for generations seems also certain, since organisms or their parts can continue almost unchanged for untold ages. That they can act independently, can be separately distributed into different germ-cells, and can be re-combined seems likewise to have been proved by the brilliant work of Mendel and his followers. So independent and constant do they appear to be that modern students of heredity tend to treat them as so many beads in a row, as separate particles themselves endowed with all the properties of independent living organisms, the very properties we wish to explain. While not prepared to accept these views without qualification, it seems to me that it can scarcely be doubted that some such units must exist whether in the form of discrete particles or merely of separable substances. But not until these factors have been brought into relation with the general metabolism of the organism, as links in the chain of processes, will the problem of inheritance approach solution. If the theory is to be completed it must attempt to explain how they come to differ, how their orderly behavior is regulated, in what functional relation they stand to each other, what is the metabolic bond between them. That harmonious processes may be carried out by discrete elements in cooperation is shown in cases of symbiotic combinations such as the lichens, or the green algæ in such animals as Hydra and Convoluta. Here an originally independent organism takes its place and does its work regularly in another organism, and may even be propagated and transmitted from one generation to the next in the germ-cell! Most instructive, also, are the recently studied cases of bacteria and yeasts living regularly in certain special tissues of various species of insects, where they exert a definite influence on the metabolism (see the works of Pierantoni, Buchner, Gla-

[7] Herbert Spencer's "physiological units," Darwin's "pangens," Weismann's "determinants," are all terms denoting factors, but with somewhat different meanings. More recently Professor W. Johannsen ("Elemente der exakten Erblichkeitslehre," 1909) has proposed the term "gene" for a factor, "genotype" for the whole assemblage of factors transmitted by a species, and "phenotype" for the characters developed from them. This clear system of nomenclature, although much used in America, has not been generally adopted in this country.

ser). These no doubt are mere analogies, but they serve.

In all probability, then, factors of inheritance exist, and the fundamental problem of Biology is, how are the factors of an organism changed, or how does it acquire new factors? In spite of its vast importance, it must be confessed that little advance has been made towards the solution of this problem since the time of Darwin, who considered that variation must ultimately be due to the action of the environment. This conclusion is inevitable, since any closed system will reach a state of equilibrium and continue unchanged, unless affected from without. To say that mutations are due to the mixture or reshuffling of preexisting factors is merely to push the problem a step farther back, for we must still account for their origin and diversity. The same objection applies to the suggestion that the complex of factors alters by the loss of certain of them. To account for the progressive change in the course of evolution of the factors of inheritance and for the building up of the complex it must be supposed that from time to time new factors have been added; it must further be supposed that new substances have entered into the cycle of metabolism, and have been permanently incorporated as self-propagating ingredients entering into lasting relation with preexisting factors. We are well aware that living protoplasm contains molecules of large size and extraordinary complexity, and that it may be urged that by their combination in different ways, or by the mere regrouping of the atoms within them, an almost infinite number of changes may result, more than sufficient to account for the mutations which appear. But this does not account for the building up of the original complex. If it must be admitted that such a building process once occurred, what right have we to suppose that it ceased at a certain period? We are driven, then, to the conclusion that in the course of evolution new material has been swept from the banks into the stream of germ-plasm.

If one may be allowed to speculate still further, may it not be supposed that factors differ in their stability?—that whereas the more stable are merely bent, so to speak, in this or that direction by the environment, and are capable of returning to their original condition, as a gyroscope may return to its former position when pressure is removed, other less stable factors may be permanently distorted, may have their metabolism permanently altered, may take up new substance from the vortex, without at the same time upsetting the system of delicate adjustments whereby the organism keeps alive? In some such way we imagine factorial changes to be brought about and mutations to result.

Let it not be thought for a moment that this admission that factors are alterable opens the door to a Lamarckian interpretation of evolution! According to the Lamarckian doctrine, at all events in its modern form, a character would be inherited after the removal of the stimulus which called it forth in the parent. Now of course, a response once made, a character once formed, may persist for longer or shorter time according as it is stable or not; but that it should continue to be produced when the conditions necessary for its production are no longer present is unthinkable. It may, however, be said that this is to misrepresent the doctrine, and that what is really meant is that the response may so react on and alter the factor as to render it capable of producing the new character under the old conditions. But is this interpretation any more credible than the first?

Let us return to the possible alteration of factors by the environment. Unfortunately there is little evidence as yet on this point. In the course of breeding experiments the occurrence of mutations has repeatedly been observed, but what led to their appearance seems never to have been so clearly established as to satisfy exacting critics. Quite lately, however, Professor M. F. Guyer, of Wisconsin, has brought forward a most interesting case of the apparent alteration at will of a factor or set of factors under definite well-controlled conditions.[8] You will re-

[8] *American Naturalist*, Vol. LV., 1921; *Jour. of Exper. Zoology*, Vol. XXXI., 1920.

member that if a tissue substance, blood-serum, for instance, of one animal be injected into the circulation of another, this second individual will tend to react by producing an anti-body in its blood to antagonize or neutralize the effect of the foreign serum. Now Professor Guyer's ingenious experiments and results may be briefly summarized as follows. By repeatedly injecting a fowl with the substance of the lens of the eye of a rabbit he obtained anti-lens serum. On injecting this "sensitized" serum into a pregnant female rabbit it was found that, while the mother's eyes remained apparently unaffected, some of her offspring developed defective lenses. The defects varied from a slight abnormality to almost complete disappearance. No defects appeared in untreated controls, no defects appeared with non-sensitized sera. On breeding the defective offspring for many generations these defects were found to be inherited, even to tend to increase and to appear more often. When a defective rabbit is crossed with a normal one the defect seems to behave as a Mendelian recessive character, the first generation having normal eyes and the defect reappearing in the second. Further, Professor Guyer claims to have shown that the defect may be inherited through the male as well as the female parent, and is not due to the direct transmission of anti-lens from mother to embryo *in utero*.

If these remarkable results are verified, it is clear that an environmental stimulus, the anti-lens substance, will have been proved to affect not only the development of the lens in the embryo, but also the corresponding factors in the germ-cells of that embryo; and that it causes, by originating some destructive process, a lasting transmissible effect giving rise to a heritable mutation.

Professor Guyer, however, goes farther, and argues that, since a rabbit can also produce anti-lens when injected with lens substance, and since individual animals can even produce anti-bodies when treated with their own tissues, therefore the products of the tissues of an individual may permanently affect the factors carried by its own germ-cells. Moreover he asks, pointing to the well-known stimulative action of internal secretions (hormones and the like), if destructive bodies can be produced, why not constructive bodies also? And so he would have us adopt a sort of modern version of Darwin's theory of pangenesis, and a Lamarckian view of evolutionary change.

But surely there is a wide difference between such a poisonous or destructive action as he describes and any constructive process. The latter must entail, as I tried to show above, the drawing of new substances into the metabolic vortex. Internal secretions are themselves but characters, products (perhaps of the nature of ferments) behaving as environmental conditions, not as self-propagating fatcors, molding the responses, but not permanently altering the fundamental structure and composition of the factors of inheritance.

Moreover, the early fossil vertebrates had, in fact, lenses neither larger nor smaller on the average than those of the present day. If destructive anti-lens had been continually produced and had acted, its effect would have been cumulative. A constructive substance must, then, have also been continually produced to counteract it. Such a theory might perhaps be defended; but would it bring us any nearer to the solution of the problem?

The real weakness of the theory is that it does not escape from the fundamental objections we have already put forward as fatal to Lamarckism. If an effect has been produced, either the supposed constructive substance was present from the first, as an ordinary internal environmental condition necessary for the normal development of the character, or it must have been introduced from without by the application of a new stimulus. The same objection does not apply to the destructive effect. No one doubts that if a factor could be destroyed by a hot needle or picked out with fine forceps the effects of the operation would persist throughout subsequent generations.

Nevertheless, these results are of the greatest interest and importance, and, if corrob-

orated, will mark an epoch in the study of heredity, being apparently the first successful attempt to deal experimentally with a particular factor or set of factors in the germplasm.

There remains another question we must try to answer before we close, namely, "What share has the mind taken in evolution?" From the point of view of the biologist, describing and generalizing on what he can observe, evolution may be represented as a series of metabolic changes in living matter molded by the environment. It will naturally be objected that such a description of life and its manifestations as a physico-chemical mechanism takes no account of mind. Surely, it will be said, mind must have affected the course of evolution, and may indeed be considered as the most important factor in the process. Now, without in the least wishing to deny the importance of the mind, I would maintain that there is no justification for the belief that it has acted or could act as something guiding or interfering with the course of metabolism. This is not the place to enter into a philosophical discussion on the ultimate nature of our experience and its contents, nor would I be competent to do so; nevertheless, a scientific explanation of evolution can not ignore the problem of mind if it is to satisfy the average man.

Let me put the matter as briefly as possible at the risk of seeming somewhat dogmatic. It will be admitted that all the manifestations of living organisms depend, as mentioned above, on series of physico-chemical changes continuing without break, each step determining that which follows; also that the so-called general laws of physics and of chemistry hold good in living processes. Since, so far as living processes are known and understood, they can be fully explained in accordance with these laws, there is no need and no justification for calling in the help of any special vital force or other directive influence to account for them. Such crude vitalistic theories are now discredited, but tend to return in a more subtle form as the doctrine of the interaction of body and mind, of the influence of the mind on the activities of the body. But, try as we may, we can not conceive how a physical process can be interrupted or supplemented by non-physical agencies. Rather do we believe that to the continuous physico-chemical series of events there corresponds a continuous series of mental events inevitably connected with it; that the two series are but partial views or abstractions, two aspects of some more complete whole, the one seen from without, the other from within, the one observed, the other felt. One is capable of being described in scientific language as a consistent series of events in an outside world, the other is ascertained by introspection, and is describable as a series of mental events in psychical terms. There is no possibility of the one affecting or controlling the other, since they are not independent of each other. Indissolubly connected, any change in the one is necessarily accompanied by a corresponding change in the other. The mind is not a product of metabolism as materialism would imply, still less an epiphenomenon or meaningless by-product as some have held. I am well aware that the view just put forward is rejected by many philosophers, nevertheless it seems to me to be the best and indeed the only working hypothesis the biologist can use in the present state of knowledge. The student of biology, however, is not concerned with the building up of systems of philosophy, though he should realize that the mental series of events lies outside the sphere of natural science.

The question, then, which is the more important in evolution, the mental or the physical series, has no meaning, since one can not happen without the other. The two have evolved together *pari passu*. We know of no mind apart from body, and have no right to assume that metabolic processes can occur without corresponding mental processes, however simple they may be.

Simple response to stimulus is the basis of all behavior. Responses may be linked together in chains, each acting as a stimulus to

start the next; they can be modified by other simultaneous responses, or by the effects left behind by previous responses, and so may be built up into the most complicated behavior. But, owing to our very incomplete knowledge of the physical-chemical events concerned, we constantly, when describing the behavior of living organisms, pass, so to speak, from the physical to the mental series, filling up the gaps in our knowledge of the one from the other. We thus complete our description of behavior in terms of mental processes we know only in ourselves (such as feeling, emotion, will) but infer from external evidence to take place in other animals.

In describing a simple reflex action, for instance, the physico-chemical chain of events may appear to be so completely known that the corresponding mental events are usually not mentioned at all, their existence may even be denied. On the contrary, when describing complex behavior when impulses from external or internal stimuli modify each other before the final result is translated into action, it is the intervening physico-chemical processes which are unknown and perhaps ignored, and the action is said to be voluntary or prompted by emotion or the will.

The point I wish to make, however, is that the actions and behavior of organisms are responses, are characters in the sense described in the earlier part of this address. They are inherited, they vary, they are selected, and evolve like other characters. The distinction so often drawn by psychologists between instinctive behavior said to be inherited and intelligent behavior said to be acquired is as misleading and as little justified in this case as in that of structural characters. Time will not allow me to develop this point of view, but I will only mention that instinctive behavior is carried out by a mechanism developed under the influence of stimuli, chiefly internal, which are constantly present in the normal enviromental conditions, while intelligent behavior depends on responses called forth by stimuli which may or may not be present. Hence, the former is, but the latter may or may not be inherited. As in other cases, the distinction lies in the factors and conditions which produce the results. Instinctive and intelligent behavior are usually, perhaps always, combined, and one is not more primitive or lower than the other.

It would be a mistake to think that these problems concerning factors and environment, heredity and evolution, are merely matters of academic interest. Knowledge is power, and in the long run it is always the most abstruse researches that yield the most practical results. Already, in the effort to keep up and increase our supply of food, in the constant fight against disease, in education, and in the progress of civilization generally, we are beginning to appreciate the value of knowledge pursued for its own sake. Could we acquire the power to control and alter at will the factors of inheritance in domesticated animals and plants, and even in man himself, such vast results might be achieved that the past triumphs of the science would fade into insignificance.

Zoology is not merely a descriptive and observational science, it is also an experimental science. For its proper study and the practical training of students and teachers alike, well-equipped modern laboratories are necessary. Moreover, if there is to be a useful and progressive school contributing to the advance of the science, ample means must be given for research in all its branches. Life doubtless arose in the sea, and in the attempt to solve most of the great problems of biology the greatest advances have generally been made by the study of the lower marine organisms. It would be a thousand pities, therefore, if Edinburgh did not avail itself of its fortunate position to offer to the student opportunities for the practical study of marine zoology.

In his autobiography, Darwin complains of the lack of facilities for practical work—the same need is felt at the present time. He would doubtless have been gratified to see the provision made since his day and the excellent use to which it has been put; but what seems adequate to one generation becomes insuffi-

cient for the next. We earnestly hope that any appeal that may be made for funds to improve this Department of Zoology may meet with the generous response it certainly deserves.

EDWIN S. GOODRICH

THE SPIRIT OF RESEARCH

THE recent World War emphasized the importance of scientific investigation and as a result there has followed a vigorous campaign to promote research in America. In consequence a great deal has been published recently concerning the *mechanism* of research; how we may cooperate; how the large university with superior equipment may help the teacher in the small institution to keep alive the hope that is within him to do research work; we have bulletins issued from time to time which bring certain fields of knowledge up-to-date; we have compendia on the technique of research; in a host of different ways the machinery for doing research is being cleaned and oiled and must run infinitely better than it has in the past. This is all exceeedingly important and must be done if we are to take a share in the program of scientific investigation. Back of all this machinery, however, must be human minds and the progress we make in the search for truth is going to depend on the *spirit* which animates these human minds guiding this machinery of research and taking part in the actual investigation of the many unsolved problems about us and trying to

Read the world's old riddles well.

In other words, the motives which prompt men to spend long hours and sleepless nights trying to fathom the depths of the unknown will determine the success individuals have in their work.

As one goes over the records of human achievements in history, there is developed in the reader a sense that the great achievements of the world have been in the realm of the spiritual. (Using that term in its broadest meaning.) The Magna Charta, the advent of the Pilgrim Fathers, the Boston Tea Party, the Declaration of Independence, the Emancipation Proclamation are events and articles having the greatest spiritual significance. Great because they were staged for the uplift of the masses and not for the aggrandizement of the few as the failures of Alexander, Napoleon and William the Second are glaring examples.

It would seem that lessons of immense value to us might be gleaned from history as an aid in stimulating the spirit of research. What have the ancients to offer us? If achievement comes by means of spiritual forces then the animus of research must be spiritualized. Too much have we strayed from the simplicity of spirit which ruled the mind of the savant on the isle of Penikese who had

come in search of truth
Trying with uncertain key
Door by door of mystery.

Too much have we been stimulated by personal ambition in our "search for truth." Promotion, because of the amount of research we do is not the spiritualization sought for in this plea. The fundamental virtue of the investigator is a passion for truth whatever it be and through whatever channels it may come. As Bosworth says,

> One's only safety consists in a fair treatment of facts. One fact fairly treated leads to another, and this to another. Facts treated as they ought to be treated lead always to a larger life.

This means not only a larger life for the investigator but more particularly for the great human family about him. Imbued with this spirit the seeker after truth goes in its search with the altruistic ambition of making the world a better place to live in, in every sense of the word "making it safe for democracy."

Not of the sunlight,
Not of the moonlight,
Not of the starlight!
O young Mariner,
Down to the haven
Call your companions,
Launch your vessel,
And crowd your canvas,
And, e'er it vanishes

Over the margin,
After it, follow it,
Follow the gleam.

There is a grave danger for the spirit of research when the chief criterion for the advancement of an individual in his position is his ability to turn out voluminous material describing his experiments. This motive prompting the researcher tends more and more to satisfy personal ambition. There will gradually appear a greater amount of polemical writing and controversies over priority of discovery. Nor is this all or the worst of the results attained by such a stimulus to research. Inaccuracies and carelessness in obtaining results are inevitable, it is the logical outcome of a system where bulk and not quality weighs so heavily in seeking promotion. This tendency we are all aware of, not only in individuals but we recognize it as characteristic of nations as well. After all what difference does it make through whom truth is revealed if all can enjoy its fruits?

On the other hand, that land whose cricket and other sports have imbued its citizens with a sense of the "sport for the game's sake" has contributed a succession of epoch makers in the field of science that makes one wonder whence the inspiration of it all. One can not imagine the immortal Newton worrying very much about the status of his position because the first computation concerning the force of gravity due to the earth at the moon did not yield results as he had anticipated. To him and a great host of his fellow countrymen succeeding him it was sufficient to seek first the kingdom of truth, leaving it to others to judge whether the honors of earth, if they had any value, would be added as a natural result of ability. Is it not worth while for us of America, young in the research field, to consider seriously the motives which are to prompt our endeavors in the search for truth? The first motive leads to mediocre results while the latter is characterized by those discoveries which are epoch making. Shall personal ambition or the desire to be "a friend to man" surge through our endeavors? One class who followed the gleam of truth was hypocritical, men who seemed to have, and wished to seem to have the prestige of scientific distinction without actually possessing it. The other class adopted as their ideal those words which must be the true sentiment of every creative worker in every field of human knowledge:

And only the Master shall praise us, and only the Master shall blame;
And no one shall work for money, and no one shall work for fame;
But each for the joy of working, and each, in his separate star,
Shall draw the Thing as he sees It for the God of Things as they are."

S. R. WILLIAMS
OBERLIN COLLEGE,
OBERLIN, OHIO

THE CONCENTRATION OF HYDROGEN IONS IN THE SOIL

A PAPER with the above title has been published in Danish in the reports from the Carlsberg Laboratory (Meddelelser fra Carlsberg Laboratoriet), Vol. 15, Nr. 1. An English edition of this paper will soon be published in *Comptes-Rendus des Travaux du Laboratoire Carlsberg,* Vol. 15, Nr. 1.

The paper contains an account of researches carried on during the years 1916 and 1920 in order to ascertain the importance of the concentration of hydrogen ions with regard to the natural distribution of plants. Analyses were made of a series of Danish plant formations with regard to their botanical constitution, and at the same time samples of the soil were taken from the places in question, and the concentration of hydrogen ions determined. In natural Danish soil it was found to vary from 3.4 to 8.0 as expressed in pH values.

When comparing the botanical analysis of the formations with the physico-chemical analysis of the soil it was immediately seen that there is rather a fixed and constant relationship between the constitution of the vegetation and the concentration of hydrogen ions in the soil, because important variations of the latter are always accompanied by vari-

ations of the constitution of the vegetation when the other factors remain the same, whereas habitats with about the same concentration of hydrogen ions and equal with regard to light and moisture carry about the same vegetation. When the material collected was statistically investigated, it was further proved that many species are only found on soil where the concentration of hydrogen ions is within a certain range of concentration of hydrogen ions characteristic for each single species. Within this is found another range with narrower limits, within which the species has its largest average frequency. It was further proved that it was possible to judge of the concentration of hydrogen ions in the soil from the constitution of the plant formations, when they did not consist of too few species; this holds good, for instance, for meadows.

The number of species found and the density of species (the number of species found on 0.1 sq. m.) were on the whole largest on soil near the neutral point; number of species and density of species become generally less as the concentration of hydrogen ions in the soil increases.

By a series of water-culture experiments it was proved that the species which are found only on very acid soil (acid soil plants) show the strongest growth in culture media with pH values near 4, whereas species which naturally grow only in soils that are neutral or but slightly acid or basic (alkaline soil plants) have the strongest growth in culture media, the pH values of which are between 6 and 7. In the slightly acid culture media in which the basic soil plants have their strongest growth the acid soil plants thrive badly and become chlorotic.

According to the theory of Hartwell and Pember[1] basic soil plants can not thrive in very acid soils, not because these plants can not stand so high a concentration of hydrogen ions as the acid soil plants, but because the very acid soils contain small quantities of dissolved aluminum compounds, which are said to be poisonous for the basic soil plants and not for the acid soil plants. This theory has been proved not to be generally valid, as experiments have shown that aluminum ions are not poisonous for all basic soil plants, generally speaking.

According to Bear[2] and others acid soil plants can make use of the nitrogen in ammonia, whereas basic soil plants require nitrate nitrogen, which makes it impossible for them to thrive in very acid soil in which nitrification is weak or wanting. Experiments showed that nitrogen from ammonia and from nitrate nitrogen are of the same value for acid soil plants and for basic soil plants, when the plants were cultivated at constant pH. If on the other hand the pH is not kept constant, the plants make the solution more acid, when the source of nitrogen is a salt of ammonia (including thereby ammonia). In this case the basic soil plants soon die, because the solution becomes too acid. The acid soil plants on the other hand last longer as they are more tolerant of acid. If the source of nitrogen is a nitrate (nitrate of ammonia excepted), the plants make the solution more alkaline and the plants die, after having first become chlorotic. The chlorosis takes place for acid soil plants when the pH value of the culture medium has the culture medium has reached a pH value reached 6.0, but for basic soil plants not till of about 7.0.

The investigations prove that the quantity of nutritive substances does not largely influence the distribution of plants. This is opposed to the results of some investigators, who consider that the acid soils are poor and the neutral and basic soils rich in such substances. It has been proved that basic soils exist which are very poor in nutritive substances, and their vegetation does not resemble that of very acid soils, which are poor in nutritive substances.

[1] Hartwell, B. L., and Pember, F. R., 1918, "The presence of aluminum as a reason for the difference in the effect of so-called acid soil on barley and rye," *Soil Science*, 6, 259.

[2] Bear, F. E., 1917, "A correlation between bacterial activity and lime requirement of soils," *Soil Science*, 4, 435.

It is therefore probable that the concentration of hydrogen ions of the soils has a direct rather than an indirect influence on the constitution of the vegetation.

CARSTEN OLSEN
THE CARLSBERG LABORATORY,
COPENHAGEN

THE PRESENT STATUS OF THE CONCILIUM BIBLIOGRAPHICUM

PROFESSOR HENRY WARD'S appreciative account of Dr. H. H. Field and his self-sacrificing work in connection with the founding and maintenance of the Concilium Bibliographicum suggests to me to make a brief statement concerning the present status of the Concilium.

I spent several weeks in July and August of this summer in a personal examination, in Zurich, of Concilium affairs, representing the National Research Council and the Rockefeller Foundation. The Council has had for some time, during the latter months of Dr. Field's life-time and since his death, in consideration the possibility of extending some aid for the maintenance and further development of the Concilium. The Foundation has manifested a similar interest with a tangible expression of it by two appropriations to assist in meeting the current expenses of the Concilium in 1920 and 1921.

On arrival in Zurich I found Concilium matters in a critical situation. Dr. Field's patriotic activities during the war had left him but little time to devote to the Concilium, and the disastrous results of war-time and after-war conditions on such international organizations as the Concilium had left things in very bad shape. Dr. Field's sudden death prevented him from even beginning a serious rehabilitation of Concilium work and finances.

After many conferences with Mrs. Field and her business friends, with Fraülein Rühl who for twenty years has been Dr. Field's chief technical assistant and was practically the only member of the Concilium staff still giving full time to its affairs, and with an official representative of the Swiss Natural Science Association, which under the terms of Dr. Field's will becomes, under certain conditions, the legatee of Dr. Field's financial interest in the Concilium, and after long and difficult examination of the business books and memoranda of the Concilium, I arranged to set up a provisional reorganization of the Concilium under the acting directorship, until January 1, 1922, without salary, of Professor J. Strohl, of the Zoological Institute of the University of Zurich.

This temporary reorganization will allow some of the most needed work of the Concilium to go forward, supported financially by the subsidies of the Swiss Government, the city of Zurich and the Rockefeller Foundation.

The Concilium, which from the business point of view, is a non-profit taking company, most of whose shares belong to the Field estate, owns an equity of some value in the building at 79 Hofstrasse which for several years has been the Concilium offices and printing rooms. It also has some assets in the way of many already printed cards, some little stock of paper, some office furniture, type and printing presses, etc. But most importantly its assets are its "good will" and subscription list. This list must have immediate attention and revision and that is part of the work now being done under the provisional arrangement.

Professor Ward and other American biologists may be assured that the Concilium is not being allowed to go to pieces without some positive efforts being exerted to save it. It is not yet time, but soon will be, for a definite statement to be issued to the American subscribers to the Concilium cards, which, I hope, will not have to include a direct appeal for money for the support of the Concilium but will appeal for a renewed interest in, and support of the organization, to be manifested by a confirmation of old subscriptions and an addition of new ones. I was much interested to discover from examination of the subscription lists that one third of all the Concilium subscribers are American.

VERNON KELLOGG
NATIONAL RESEARCH COUNCIL

SCIENTIFIC EVENTS

THE HIGH ALTITUDE EXPEDITION TO PERU

As has been already noted in Science, the Royal Society High Altitude Expedition to Peru sailed in the third week of November on the *Santa Teresa*. The expedition proposes to study the adaptation of man to life at or above the altitude of 14,000 ft. As compared with other localities in which this type of work has been carried out, Peru possesses certain advantages: (1) Being near the equator, the effects of altitude are less complicated by those of cold than in higher latitudes. (2) The Central Railway of Peru, the highest standard-guage railway in the world, ascends the Andes to an altitude of 15,885 ft. (3) A mining population lives and works in localities situated above 14,000 and 16,000 ft., or even higher. It is alleged, for example, that the porters at the town of Cerro de Pasco, in the Andes, raise the ores 600 ft. from the mines by carrying loads of 160 lb. of mineral many times in the day. There is probably no other population which carries on such heavy work in so rare an atmosphere. Experimental methods for the study of the circulatory and respiratory systems have advanced so much within the last ten or twenty years that the time seems ripe for their application to the extraordinarily interesting problems which life at high altitudes presents. Donations towards the expenses of the expedition have been received from the folowing: The Royal Society, the Harvard Medical School, the Carnegie Fund, the Moray Fund, the University of Toronto, the Rockefeller Institute, the Presbyterian Hospital, New York, Sir Peter Mackie, and Sir Robert Hadfield.

Members of the party are Alfred C. Redfield, assistant professor of physiology at the Harvard Medical School; Arlie V. Bock, M.D., of the Massachusetts General Hospital; Henry S. Forbes, M.D., now engaged in research work in industrial medicine at Harvard University; C. A. L. Binger, of the Rockefeller Institute, New York; and George A. Harrop, of the Presbyterian Hospital, New York. The expedition was organized by Joseph Bancroft of Cambridge University, England; he is accompanied also by Professor J. G. Meakins, of Edinburgh University, and Dr. Doggart of King's College, Cambridge, England. They carry with them an X-ray machine and a large amount of other medical apparatus.

After completing the studies at Cerro de Pasco, the investigators expect to spend a short time at Ticleo, on the watershed of the Andes. Ticleo, nearly 16,000 feet high, is the highest standard-gauge railroad station in the world. They will return by February first, and later in the year Mr. Bancroft will give a series of lectures at the Lowell Institute in Boston.

THE JOSEPH HENRY FUND OF THE NATIONAL ACADEMY OF SCIENCES

In the year 1878 a tripartite agreement was made between (1) Certain citizens of Philadelphia, (2) A Pennsylvania Insurance and Annuity Company and (3) the National Academy of Sciences, by the terms of which a fund of $40,000 face value was placed in trust with the Company, the income from which was to be paid to Professor Joseph Henry during his life and after his death to his wife and three daughters and after the death of the last survivor of these four, it was provided that the same gross sum shall be transferred to the National Academy of Sciences to be forever held in trust and the income from which shall be from time to time applied to assist "meritorious investigations in natural science especially in the direction of original research."

By the death on November 10, 1920, of the last survivor of the original beneficiaries, the capital sum passes, as of that date, into the hands of the National Academy of Sciences for purposes as indicated.

At the recent fall meeting of the Academy in Chicago, the following statement of policy of administration, submitted by the special Committee on this fund, was approved by the Academy:

Under the terms of the trust deed there is im-

posed no limitation regarding the field of science in which an award may be made. Since, however, this fund, in its original inception was organized during Professor Henry's life time for the purpose of enabling him the better to carry on his scientific work, and since it now stands, in some measure, as a monument to his name and to his contributions to science, it would seem not improper that among projects of equal merit otherwise, some preference should be shown to those which may lie nearer to the fields of work with which Professor Henry's name is usually associated. The committee does not, however, desire to impose in advance any specific limitations or restrictions, and it will therefore be prepared to consider applications from all fields of natural science.

It is probable that a certain amount of money may be avaliable for award at the meeting in April next. Applications for award should be forwarded to the Secretary of the National Academy of Sciences, Smithsonian Institution, Washington, D. C., on or before April 5, 1922.

Suggestions regarding the general problem of the most effective utilization of such a fund will be gratefully received by the chairman of the committee.

W. F. DURAND,
Chairman, Joseph Henry Fund Committee

STANFORD UNIVERSITY,
CALIFORNIA

DR. NICHOLS AND THE PRESIDENCY OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY

DR. ERNEST FOX NICHOLS, president of the Massachusetts Institute of Technology, has resigned his office because of ill health and his resignation has been accepted by the executive committee of the corporation. He has been given leave of absence until January 4, 1922, when the next meeting of the corporation will be held and the action of the executive committee will be ratified. Dr. Nichols was inaugurated president of the institute on June 8, 1921, but has not assumed the office.

Dr. Nichols's letter to the corporation follows:

A sufficient time has now elapsed since the onset of a severe illness, which followed immediately upon my inauguration, to enable my physicians to estimate consequences. They assure me certain physical limitations, some of them probably permanent, have resulted. These, they agree, make it decidedly inadvisable for the institute or for me that I should attempt to discharge the manifold duties of president. Indeed, they hold it would be especially unwise for me to assume the grave responsibilities, to attempt to withstand the inevitable stresses and strains of office, or to take on that share in the open discussion of matters of public interest and concern inseparable from the broader activities of educational leadership.

As my recuperation is still in progress I have contended earnestly with my doctors for a lighter judgment. I feel more than willing to take a personal risk, but they know better than I, and they stand firm in their conclusions.

The success of the institute is of such profound importance to our national welfare, to the advancement of science and the useful arts, that no insufficient or inadequate leadership is sufferable. Personal hopes and wishes must stand aside.

It is therefore with deep personal regret but with the conviction that it is best for all concerned, that I tender you my resignation of the presidency of the institute and urge you to accept it without hesitation.

To you who have shown me such staunch and generous friendship it is pleasant to add that in the judgment of my physicians the physical disqualifications for the exigencies of educational administration are such as need not restrict my activities in the simpler untroubled, methodical life of scientific investigation to which I was bred. It is to the research laboratory, therefore, that I ask your leave to return.

In reply Frederick P. Fish, chairman of the executive committee of the corporation, wrote as follows:

Your letter of November 3, 1921, to the Corporation of the Massachusetts Institute of Technology was submitted to the executive committee of the institute at a meeting of the committee on November 10, 1921.

The situation set out in your letter is clearly controlling and the committee had no alternative except to accept your resignation, subject to confirmation by the corporation. As appears by the vote of the committee, copy of which I enclose, your resignation is to take effect January 4, 1922, with leave of absence until that date.

I can not adequately express the deep regret of the committee that the institute must lose your services as its president. We have all been looking forward with the utmost confidence to the sound development and continued prosperity of the institution under your leadership. We have no doubts as to the future but shall never cease to deplore that you were not permitted to make the great contribution to the work which your character, personality and training would have assured to it.

I need not add that the severance of the personal relations which have given us so much satisfaction is a source of keen regret to us all. We know, however, that you will always remain a friend of the institute and of those who are responsible for the guidance of its affairs.

The members of the committee and the friends of the institute generally, will cordially unite in wishing you a long, happy and prosperous life and large success in the work to which you propose to devote your effort.

MEETINGS OF NATIONAL SCIENTIFIC SOCIETIES

REDUCED railroad fares for those attending the Toronto meeting of the American Association for the Advancement of Science (December 27 to 31) have now been granted by the Southeastern, Western and Southwestern Passenger Associations, as well as by those named in a recent announcement (SCIENCE, 54: 353, October 14, 1921). Every member planning to attend the meeting from the regions of the Transcontinental Passenger Association should consult his local ticket agent, and purchase a ticket to the nearest main station lying within the region for which the reduced rates are available. The complete list of passenger associations granting the reduced rates is: The Canadian Passenger Association, The New England Passenger Association, The Central Passenger Association, The Southeastern Passenger Association, The Western Passenger Association, and the Southwestern Passenger Association. The rate from main stations within the regions of these associations will be a fare and one half for the round trip, on the certificate plan.

THE next meeting of the American Astronomical Society will be held on December 29–31, at Sproul Observatory, Swarthmore, Pa.

THE Ecological Society of America will hold its annual meeting at Toronto in affiliation with the American Association from December 27–30. In addition to the regular sessions of the society joint sessions will be held with the Entomological Society of America, the American Society of Zoologists and the Botanical Society of America. Members wishing to present papers should furnish the secretary with titles and brief abstracts as soon as possible. The society headquarters will be at the King Edward Hotel. Communications in regard to participation in the program and in regard to membership should be sent to the secretary, A. O. Weese, The Vivarian, Champaign, Illinois.

THE annual meeting of the Federation of American Societies for Experimental Biology, composed of the American Physiological Society, The American Society of Biological Chemists, The American Society for Pharmacology and Experimental Therapeutics, and The American Society for Experimental Pathology, will be held in New Haven under the auspices of Yale University on December 28, 29, and 30. The American Association of Anatomists will meet at the same date and place. The advantage of one and one half round trip fare on the certificate plan has already been granted by the railroads of the territory east of Chicago and St. Louis and south of the Canadian border. These rates are available to members and their friends attending the annual session. The federation meeting is under the executive chairmanship of Dr. J. J. R. MacLeod, of the University of Toronto, president of the American Physiological Society.

THE annual meeting of the Association of American Geographers, under the direction of President Ellen Churchill Semple, will be held in Washington, D. C., on December 29, 30 and 31, beginning on Thursday at one thirty. Through the courtesy of the National Geographic Society the session will be held at the society building. Morning sessions Friday and

Saturday will extend from ten to one o'clock; afternoon sessions Friday and Saturday from two thirty to five thirty. The president's address will be given at the opening of the session on Friday afternoon, and will be followed by a series of invited papers on "Trade Routes."

The American Society of Mechanical Engineers will hold its annual meeting in New York city from December 5 to 9. The report of the committee on elimination of waste in industry of the American Engineering Council will provide the basis for the discussion.

SCIENTIFIC NOTES AND NEWS

The Norwegian Störthing has awarded the Nobel peace prize for 1921 to Dr. Elis Stroemgren, professor of astronomy at the University of Copenhagen, for his efforts to effect reconciliation among scholars of European countries.

Dr. T. C. Chamberlin, of the University of Chicago, has been made a corresponding member of the Stockholm and Belgian Geological Societies.

Dr. Simon Flexner, the director of the Rockefeller Institute for Medical Research, New York, has been elected a corresponding member of the Vienna Society of Physicians.

Professor George Grant MacCurdy, of Yale University, first director of the American School in France for Prehistoric Studies, has been elected a corresponding member of the Société Archéologique et Historique de la Charente.

Dr. John B. Whitehead, dean of the engineering school and professor of electrical engineering at Johns Hopkins University, has been awarded the five thousand francs prize of the Institute Electrotechnique Montefiore of Liège, Belgium, bestowed every three years for original work on the scientific advancement in the technical application of electricity. The prize was given for an essay on "The Corona Voltmeter and the Electric Strength of Air."

The Jenner Memorial Medal of the Royal Society of Medicine has been conferred on Sir Shirley Murphy in recognition of his work in epidemiological research.

The University of Cambridge has presented an address to Dr. G. D. Liveing, St. John's College, formerly professor of chemistry, to commemorate the fact that he has kept by residence every term in the university for the last seventy-five years. Dr. Liveing became fellow of St. John's College in 1853, and professor of chemistry in 1861.

President Livingston Farrand, of Cornell University, was elected president of The American Child Hygiene Association at its annual convention in New Haven, on November 5.

Professor Filibert Roth, head of the department of forestry of the University of Michigan, was recently appointed by Governor Groesbeck as a member of the State Commission of Conservation. Professor Roth represents on the commission the forestry interests of the state.

David Lumsden, formerly assistant professor of floriculture at Cornell University and during the last two years director of Agricultural Reconstruction at Walter Reed General Hospital, has been appointed horticulturist in the Office of Foreign Plant Quarantines, Federal Horticultural Board, Washington, D. C.

Messrs. J. E. Walters, F. W. Schroeder, and Frank Porter, chemists at the helium plant of the Bureau of Mines at Petrolia, Texas, have been transferred to the new cryogenic laboratory of the bureau in Washington.

Mr. Earle E. Richardson, who has been instructing in analytical chemistry and physics for the past four years at the Massachusetts Institute of Technology, has been appointed research physicist under Mr. L. A. Jones at the research laboratories of the Eastman Kodak Co., Rochester, N. Y.

Mr. Allen Abrams has resigned as research associate from the research laboratory of applied chemistry at the Massachusetts Institute of Technology to become chief chemist for the Cornell Wood Products Co.

Dr. L. I. Shaw, assistant chief chemist of the Bureau of Mines, has been transferred to the Columbus, Ohio, ceramic experiment station of the bureau, where he will have charge of some newly organized research on refractory products.

Wilson Popenoe, agricultural explorer for the U. S. Department of Agriculture, has returned to Washington after a two years' absence in Guatemala, Costa Rica, Colombia, Ecuador, Peru and Chile. Mr. Popenoe has sent to Washington from these countries living material of numerous food-plants, including new varieties of the avocado for trial in California and Florida, several promising species of Rubus, the pejibaye palm (*Guilielma utilis*) of Costa Rica, a collection of potatoes from Ecuador and Colombia, and a superior variety of the Andean cherry (*Prunus salificolia*).

Professor Franz Doflein, now at the Zoological Institute at Breslau, Germany, is completing a revision of his "Lehrbuch der Protozoenkunde." He finds it difficult to secure in Germany access to American papers in the field of protozoology published since 1916 and will welcome the sending, from investigators in this field, of reprints of their papers.

Professor Henry Norris Russell, of Princeton University, spoke before the Physical Colloquium of the Western Electric Company in New York, recently, on the subject "Ionization in the Stars."

Professor J. H. Walton, of the department of chemistry of the University of Wisconsin, lectured before the Milwaukee Section of the American Chemical Society on November 18 on "The influence of impurities on the rate of growth of certain crystals."

At a joint meeting of the Washington Academy of Sciences, the Biological Society of Washington and the Botanical Society of Washington on November 12, Professor Arthur de Jaczewski, director of Institute of Mycology and Pathology at Petrograd, delivered an address on "The development of mycology and pathology in Russia"; Professor Nicholas I. Vavilov, director of the Bureau of Applied Botany and Plant Breeding at Petrograd, delivered an address on "Russian work in genetics and plant breeding," and Dr. Vernon L. Kellogg, permanent secretary of the National Research Council, led a discussion on "The interrelations of Russian and American scientists."

Dr. Heber D. Curtis, director of the Allegheny Observatory, lectured before the Franklin Institute at Philadelphia on November 16 on "The spiral nebulæ and their interpretation." On the following day he lectured before the Washington Academy of Sciences on "The sun, our nearest star."

The series of lectures on "The evolution of man" under the auspices of the Yale chapter of the Society of the Sigma Xi will include a lecture on "The evolution of intelligence" by the president of the university, Dr. James R. Angell.

The winter course of popular scientific lectures before the Royal Canadian Institute at Toronto was inaugurated on October 29 by a lecture entitled "Some aspects of economic entomology," by Dr. L. O. Howard, chief of the Bureau of Entomology of the U. S. Department of Agriculture. It is the purpose of the institute to have scientific men from the United States deliver lectures in this course during the coming season.

Professor Douglas W. Johnson, of Columbia University, delivered a lecture on the "Topography and strategy of the Western Front" before the officers of the Naval War College at Newport, on October 28. On November 1 he addressed the New York Post of the Society of American Military Engineers on "Geology and topography in relation to the strategy and tactics of the Great War."

We learn from *Nature* that the 168th session of the Royal Society of Arts will be opened on Wednesday, November 2, at 8 p.m., when Mr. Alan A. Campbell Swinton, chairman of the council, will deliver an experimental address on "Wireless telegraphy." Among the papers fixed for the meetings up to Christmas are the following: The work of the industrial fatigue research board, by D. R.

Wilson; Modern buildings in Cambridge and their architecture, by T. H. Lyon; The coming of age of long-distance wireless telegraphy and some of its scientific problems (Sir Henry Trueman Wood Lecture), by Professor J. A. Fleming; and The preservation of stone, by Noel Heaton.

AN inter-allied exhibition of hygiene will take place in Strasbourg on May 1, 1923, on the occasion of the centenary of Pasteur's birth. The commissioner general is Professor Borrel, the secretary general M. Emile Henry.

A SENATE joint resolution by Senator Heflin of Alabama would authorize that $50,000 be spent in the erection of a monument in the city of Washington to Major-General William C. Gorgas, former surgeon-general of the army, in commemoration of the services rendered by him to humanity.

RAYNER M. BEDELL, electrical engineer, brother of Professor Frederick Bedell, of Cornell University, died of tetanus on November 5, at Montclair, N. J.

DR. MERWIN PORTER SNELL, a member of the scientific staffs of the Smithsonian Institution and the Bureau of Fisheries in the years 1881–1889, died at his home at Stamford, Connecticut, on September 23, 1921, at the age of fifty-eight years.

THE death is announced on October 29 of William Speirs Bruce, the oceanographer and polar explorer.

DR. FRANCIS ARTHUR BAINBRIDGE, university professor of physiology at St. Barthomew's Hospital, died on October 27th at the age of eighty-six years.

ETIENNE BOUTROUX, professor of philosophy at the Sorbonne since 1885, died in Paris on November 22, at the age of seventy-six years. During 1910 M. Boutroux delivered a series of lectures at Harvard University.

THE death is reported from Paris, at the age of seventy-two years, of the French engineer, M. Albert Sarpiaux, who had long been connected with the scheme for the construction of a tunnel under the English Channel.

DR. PIERRE HENRI SOILLIER, honorary professor of the Lyons Medical Faculty and corresponding member of the Academie de Médecine, has died at the age of eighty-eight years.

OUR attention has been called to the fact that Dr. Emil A. Budde, whose death was announced in the issue of SCIENCE for November 18th, was president of the Electrotechnical Commission and not of the Electrochemical Commission as there stated. The succession of presidents of the Electrotechnical Commission has been Kelvin, Mascart, Elihu Thomson and Budde.

THE Royal Astronomical Society of Canada will meet in Toronto with the American Association for the Advancement of Science, and will join in the program of Section D of the association.

DISCUSSION AND CORRESPONDENCE

FUR SEALS OFF THE FARALLONS

So little is known regarding the whereabouts of the Alaska Fur Seals during the period of their absence from their breeding grounds on the Pribilof Islands, that the following definite record will be of interest.

The observations here recorded were made by Mr. John Kunder, at that time keeper of the Farallon Light Station, and communicated to me by Captain H. W. Rhodes, superintendent of lighthouses, 18th district, San Francisco.

Mr. Kunder states that on or about March 4, 1920, at 9 A.M. a herd of seals appeared about two miles due south of the Farallons. They presented a compact front line about three miles in length. They were about two miles away when first observed and were moving toward the island. They appeared to stop for a moment to gaze at the object at their front, then their left wing slowed down and the right moving rapidly, the seals jumping out of the water, the line veered around in regular military formation and a new line was formed which moved off in a west-northwest direction. After completing the new formation the herd moved very fast. The line was well-formed at all times, there being few or no stragglers.

When first seen approaching, Mr. Kunder

says the commotion in the water was like a line of breakers coming from due south toward the island, but with field glasses it was easy to determine the real cause of the disturbance. Mr. Kunder estimated the number of seals in the herd at 8,000 to 10,000.

On March 10, 1917, Mr. Kunder witnessed a similar phenomenon. This herd appeared at about five o'clock in the evening, in the same locality, and its movements, appearance, and course were about the same as with the 1920 herd. The 1917 herd was, however, considerably larger than that of 1920, the number of seals in it being estimated by Mr. Kunder at 15,000. Mr. Kunder says he has never seen any single fur seals or small groups in the vicinity of the island.

So far as I am aware this is the first record of the occurrence of the fur seals in large compact herds anywhere in the open sea; they have hitherto been observed or reported only in more or less scattered numbers.

Barton Warren Evermann
California Academy of Sciences

THE PHYSICAL MUSEUM OF THE UNIVERSITY OF WISCONSIN

So much interest has been shown in this little museum that a brief description of it in the columns of Science seems worth while. It is the outgrowth of an attempt to build up on a small scale, for the benefit of our students, a collection of simple demonstration experiments such as is exhibited in, say, the Urania of Berlin. When our new laboratory was built some four years ago we arranged for a room, in size about 18 × 40 feet, parallel to the main corridor and separated from it by a glazed partition. In this we have gradually accumulated some forty "exhibits," each with an explanatory card setting forth the theory as simply as is consistent with scientific accuracy. While many of the exhibits are of the fixed variety, *e.g.*, the parts of an ammeter, various stages of lamp bulb construction, transparencies and the like, the most interesting demonstrations, needless to say, are those which "work."

First and foremost, of course, is the Foucault pendulum, which in this case is 1440 cm. long and occupies a special well. It is started every morning at 8 o'clock and swings over a card graduated in hours (for this latitude). It is accompanied by a small rotating table of the usual demonstration variety with a miniature Foucault pendulum. A large electrically driven gyroscope mounted in a box which may be wrestled with, gives a striking demonstration of gyroscopic reactions. A loop-the-loop model, ball on stream of water, probability board (shot), Kater pendulum and simple air-pressure demonstration are among the other mechanics exhibits. There is also a conservation-of-angular-momentum rotating platform (contrived with the aid of a Ford front-wheel bearing) on which one may stand with a dumbbell in each hand and perform this somewhat startling experiment.

The Melde experiment, various Foucault current phenomena and certain magnetic effects are all susceptible of easy demonstration, as are also simple thermo-electric effects. One of the most interesting and simple optical arrangements is a pair of plane mirrors set at a right angle. In these one may—possibly for the first time—"see himself as others see him," while reflected printed matter is readable. The explanation is almost obvious. Our two most recent and pretentious exhibits—an oscillating audion circuit and a vacuum discharge demonstration—have attracted considerable attention.

The interest shown in the museum has been very gratifying. Just now, although this is its third year, the attendance is in the neighborhood of two hundred visitors a day. It is very unusual to find less than half a dozen trying the experiments and sometimes the room is literally crowded full. The wear on certain pieces of apparatus shows graphically the thousands of times they have been handled. While drawn mostly from the student body the visitors frequently include the casual outsider who comes to take a "one-hour course in physics."

It is very difficult to estimate just what good "results" may be claimed for such a

museum. Undoubtedly many come merely to toy with the apparatus, but some few pore over the explanations and ask questions about them. That it has awakened an interest in the subject in many for the first time may be taken for granted. One very definite advantage is that it allows the instructor to refer his students to certain experiments in the museum with the request that they try them and report on the results, *e.g.*, all our elementary students determine, from its period, the length of the large pendulum.

However, while it seems eminently worth while it is needless to say that such a museum, simple as it is, will not run itself. Although it does not require the presence of an attendant, its continued demand for new experiments as well as the upkeep of the old ones would constitute a perhaps unwarranted liability on the time of the instructional force of the department if it could not, as in the present case, be entirely turned over to an ingenious and able apparatus man.

L. R. INGERSOLL

MADISON, WIS.,
November 5, 1921

HOW TO DO RESEARCH [1]

I HAVE never done any research. I am therefore able to give unbiased advice regarding it.

Research—in the broadest sense—consists largely of repairing leaks in glass tubing.

More specifically, it consists of gathering in a cell down in the Ryerson basement a weird assembly of switches, wires and glass tubing—and then keeping other students from borrowing it.

Apparatus may be borrowed or acquired. If you borrow it you are expected to return it. If you acquire it, you keep it until you are found out.

Tools at one time could be found in the student's shop. Now you find them everywhere.

In order to do research, one must have ideas. *One* idea is sufficient. Two ideas are apt to contradict each other.

Ideas are easy to get. If you haven't any, consult Dr. Gale. He can be found adjusting gratings down in the basement.

By all means do *not* search for something original. If you think you have a *new* idea read Professor Groszkopf's articles in "Zeitschrift für So und So" published about 1700. You will find he suggested the same thing two centuries ago.

After all, it is doubtful whether even one idea is necessary. Merely get some apparatus, solder it together and take readings.

Readings are always taken through a telescope.

You will get certain numbers. Plot these numbers against other numbers which you get from variable parts of the apparatus.

If you get a straight line on plotting your observations you know at once that the results could have been predicted.

However, if you get a curve the situation is different. Examine the curve carefully for sharp bends or breaks. If you find one, you have made a discovery. These breaks are significant. Consider carefully what may have caused such breaks. Try to trace them to atomic or electronic phenomena. Draw a picture of the atom. Don't be discouraged if your picture doesn't agree with other pictures. Dr. Lunn will show it doesn't mean anything anyhow.

Having obtained a curve and concocted a theory, it is befitting that you present the whole to the Physics Club.

The Physics Club was invented to keep research students from getting the big head. It consists of a crowd of professional knockers. There is one booster. You are the booster.

It is fitting here to give you details on your conduct at the meeting.

The latter is always preceded by tea. While this is being served go into the lecture room and copy a few weird sketches of your apparatus on the board. Make everything as

[1] Read at a gathering of the graduate students in Physics of the Ryerson Physical Laboratory on a social occasion preceding Professor Milliken's departure from Chicago.

complicated as possible. Also prepare a few slides. They may be shown at embarrassing moments.

As soon as the club is assembled, gaze upon them with a dreamy eye and begin your talk.

The first step is to write nine long equations on the board.

Somebody will call your attention to the fact that the fifth term of the first equation should have a minus sign.

Memorize the equations beforehand if possible. Write them rapidly.

The success of your talk will depend directly on the number of people you can shake off at this point.

Mathematics is always helpful in this way. If your audience looks too intelligent, cover the board with partial derivatives and integral signs.

Having presented the equations dwell at great length on the sub-electron, the rigidity of the ether, or the density of petrified rhubarb in Siberia.

Finally when you see that vacant stare, indicative of a temporary lapse of intelligence, steal into the eyes of the front row, it is time to stop.

Pause for effect. Gather up your books—several volumes of "Annalen der Physik" and four score and seven sheets of loose notebook paper and ask for questions.

There will always be questions. They are indicative of an intelligent audience.

Then there will be a discussion. In this *you* will have no part. However, at its close you will be convinced of three things:

First: that you were entirely wrong.

Second: that you did a fine piece of work.

Third: that it doesn't mean anything.

The moral of this paper is: It is much easier to take data than to interpret the results.

A. W. Simon

SCIENTIFIC BOOKS

Organic Dependence and Disease: their Origin and Significance. By John M. Clarke. Yale University Press, 1921. Pp. 113, 105 text figs.

In a new book, marked by deep thinking, and written with Huxleian vigor and picturesqueness of phrase, we have presented to us the philosophy of righteous living as seen by a paleontologist, a life-long student of Paleozoic faunas and floras. Beginning with a study of mutual and commensal living, we are shown how this develops into parasitism, and out of it all comes to us the true significance of ease in life and dependence. Progress, racial or individual, does not lie in this direction, and once entered upon, there is no return road to independence, the only righteous mode of living.

We need not present the evidence on which Clarke's philosophy is based, since the book itself gives this so clearly, but can go at once to the conclusions. Parenthetically, however, we would advise the reader to study along with the book under review Conklin's "The Direction of Human Evolution," a most interesting work on philosophical naturalism, showing what evolution has done for man morphologically, and what in all probability social evolution will do for him. In these two books we have revealed to us the naturalist's religion as Nature has unfolded it throughout the geological ages. As Conklin says,

> The new wine of science is fermenting powerfully in the old bottles of theology.

The purpose of Clarke's essay is to set forth the apparent controls governing the historical origin of dependent and abnormal conditions of life, and from this evidence to generalize their significance to humanity. The bases of this knowledge are Paleozoic invertebrate fossils, plus the vista of organic accomplishments through untold millions of years. The evidence is presented without embarrassing detail and the conclusions without bias, and their human concerns are of high moment.

The author states that "disease is discomfort," and agrees with Huxley that "disease . . . is a perturbation of the normal activities of a living body." In other words,

Disease is *any departure from normal living*. . . . The *entire body*, organism or creature and the *entire race* or stock to which it belongs may become abnormal through subjection to an abnormal or perturbed mode of life. Such body, creature, race or stock is therefore in a state of disease.

The question, What is normal living? is answered through a study of the earliest marine faunas.

Normal living, in the broad sense in which we desire to be understood, means full activity of an unimpaired physiology inclusive of the function of locomotion or mobility. . . . Independent living, freedom of locomotion and range expose the individual to ever new dangers. These the individual must quickly overcome or outwit; otherwise succumb. The choice is quick, imperious and final. . . . Normal living is, in terms of biology, correct living, that is to say, righteous living, and in so far as dependence invades the mode of life whether in organ or individual, such living is unrighteous, disordered and diseased; in better phrase, biologically, is without hope, for such perturbation or disease is beyond voluntary or casual rectification.

Out of right or normal independent living have come all the great triumphs of life; the races of life which, by keeping individual and racial independence, have persistently climbed upward. . . . The giants of the redwood forests are the hoary and venerable obelisks of power shackled beyond redemption; the gardens of flowers are blossoms of a hope never to be attained.

In all of the evolution of endlessly variant life, there has been, however, "a strong minimum, a redeeming minority, of competent upward evolution." It is a certainty that the minorities of geologic life have saved the day for us.

Wise students of nature, in reflecting on this thought, have broken out into exclamations of wonder and amazement at the slender thread of chance by which we who call ourselves men have come to this estate, in a world where for millions of years the temptation to the easier way and the obstacles to independent living were constantly against us.

It would be trite to say that a perfectly adjusted life is an unprogressive one. The adjusted life makes for conservatism and reduces the chances of variation to its lowest terms. . . . Speaking for the moment in higher terms for the individual the adjusted life is likely to carry with it the highest content of happiness. To progress in organic development it is the undeniable foe, but to the conservatism of intellectual and spiritual ideals the undoubted friend.

Clarke finds that 90 per cent. of Cambrian organisms led a life of independence. In subsequent time, dependent life becomes ever greater in individuals and races. Interdependent individual life as expressed in mutual and commensal adaptations is sparingly present in the Ordovician but "not until life had got in full swing did these organic combinations come into existence, even in their simplest commensal expressions." Out of the innocent combination of symbiosis arises parasitism, "an adaptation in which one organism has become helplessly dependent on another for its existence."

If dependence has affected and sealed the fate of one great division of the Kingdom of Life, so that it is and must remain subsidiary to the larger purposes of nature, dependence also has entered upon, invaded and degenerated a very large part, indeed, probably the major part of the other, the animal world. . . . Dependent races of animals have sought or accepted dependence as an easier mode of living, either waiting upon the unconscious forces of Nature, waves and winds, or on the normal activities of other animals. Such dependence has entered in some degree upon all primitive stocks of animal life and from such racial dependence there has been no escape. The lines in the animal world along which links in the chain of advancement have continued unbroken, are but few; the rest have run out into culs-de-sac where all hope is abandoned.

Rescue of dependents is therefore not a part of the scheme of Nature, except through the exercise of intelligence. In Nature's plan of evolution dependents of all sorts are negligible and abandoned to hopelessness, save as gradually developing psychic factors intervene.

These conclusions are so well established that we may rightly look to them for light upon the interpretation of certain tendencies to rest and unrest, conservatism and impulsive change, in human society, and while it may not seem very appropriate to speculate on the further bearing of this theme, it must be said in looking back over the field of organic history, that the value of the product must be in terms of the worth of the type

conserved or broken; that is, worth in the sense of highest attainment in functional grade and in the approach to mentality.

CHARLES SCHUCHERT

SPECIAL ARTICLES

A SIMPLE MICRO-INJECTION APPARATUS MADE OF STEEL

FOR injection and suction purposes in the field of the compound microscope two very good methods are in existence. One is Barber's[1] mercury pipette. This consists of a glass tube completely filled with mercury. One end is bent into several loops and sealed at the tip. The other end is drawn out into a capillary with a microscopic aperture at its tip. The pipette is held in Barber's pipette holder which is clamped to the stage of the microscope. For injection and suction purposes Barber depends on the expansion and contraction of the mercury by varying the temperature of the loops of the pipette. This method gives excellent results but the pipette is rather difficult to make, it is easily broken and the driving force of the mercury can not be instantly controlled.

A more recent method is that of Taylor's,[2] which also consists of a mercury-filled pipette, but which depends upon a minute plunger to regulate the pressure of the mercury. The plunger method gives the operator a better control of the pressure in the pipette but has the disadvantage of possible leakage around the plunger. This generally occurs after the plunger has been used several times. A great deal of time tends to be wasted in keeping the apparatus in a working condition.

The apparatus described here is very simple to set up and, excepting for the few inches of capillary pipette which can be inserted into the apparatus within a few minutes, it is permanently ready for use. The apparatus depends upon leverage clamps to regulate the mercury pressure which can be controlled at any instant. Consisting entirely of steel and heavy glass it is practically unbreakable, a consideration of great importance for easy manipulation.

As in Barber's and Taylor's instruments, mercury is used to procure the necessary pressure. The apparatus consists of a thin-walled, (.028 inch or less thick), straight, one half inch, steel tube about six inches long (see figure). Into one end of this is sealed an

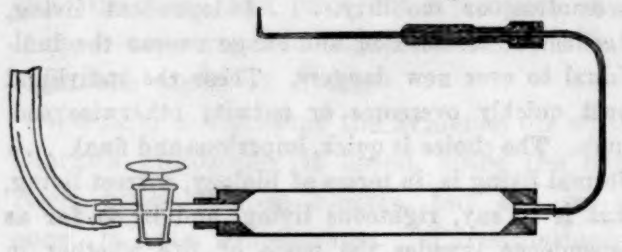

FIG. 1.

accurately fitting steel or glass stopcock. The other end leads into a small steel tube fine enough to be flexible, viz., about 3/32 of an inch in outside diameter. The small tube is bent into a twisted S shape, so that, when at rest, its tip lies on a pipette carrier on the stage of the microscope. The tip of this thin tube is furnished with a screw joint by means of which it may be attached to a hollow steel rod two inches long which carries the glass micro-pipette. The outer end of the stopcock is connected with a rubber tube about four inches long. The steel tube is placed in a special clamping device which is secured to the table beside the microscope. This clamping device consists of three leverage clamps, one of which presses on the steel tube in a direction at right angles to that of the other two.

The apparatus is first filled with clean mercury through a glass funnel inserted into the rubber tube upon which the stopcock is closed. The glass pipette is made according to Barber's method[3] and is sealed with wax into the hollow steel rod.

[1] Barber, M. A., 1911, "A technic for the inoculation of bacteria and other substances into the cavity of the living cell," *Jour. Inf. Dis.*, VIII., 348; 1914, "The pipette method," etc., *The Philip. Jour. Sc.*, Sec. B, Trop. Med., IX., 307.

[2] Taylor, C. V., 1920, "An accurately controllable micropipette," SCIENCE, N. S., LI., 617.

[3] See footnote 2, also Chambers, R., 1918, "The microvivisection method," *Biol. Bull.*, XXXIV., 121.

The rod is then screwed to the end of the tube of the injection apparatus by means of the screw point in which is a fiber washer to make the joint tight. The rod is then clamped in a mechanical pipette holder, either that of Barber or one described in an article already printed. The next step is to fill the pipette with mercury. To do this open the stopcock and see that the rubber tubing connected with the stopcock is full of mercury. With a strong clamp close the tubing about four inches from the stopcock. Along this four inches place several screw clamps which, on being screwed down, will produce sufficient pressure to drive mercury almost to the tip of the pipette. The stopcock is then to be securely shut off.

We are now ready for action. Squeezing the metal tubes by one or other of the leverage clamps will drive mercury through a pipette having an aperture of only one micron (.001 mm.) in diameter. Move the pipette by means of the pipette holder till its tip projects into a hanging drop of the solution to be injected. Release pressure on the steel tube and some of the solution will be drawn into the pipette. Now lower the pipette and move the moist chamber till the cell to be injected is brought into view. The pipette is now raised until it punctures the cell. On applying pressure to the steel tube the solution is readily injected. The apparatus may also be used to withdraw materials from the cell.

The apparatus is extraordinarily sensitive. The meniscus of the mercury in the pipette responds instantly to the pressure of the leverage clamps. A comparative estimation of the quantity of injection material used may be made by focusing, first, on the mercury meniscus, then on the tip of the pipette and measuring the distance of the two focal points by means of the fine adjustment screw of the microscope.

A more complete description of this apparatus will shortly be published.

ROBERT CHAMBERS
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ON THE EMISSION AND ABSORPTION OF OXYGEN AND AIR IN THE EXTREME ULTRA-VIOLET

UP to this time very little has been known of the spectrum of oxygen in the region of wave-lengths shorter than λ2000. Some previous investigators were unable to obtain a spectrum in this region. "No lines or bands," says Lyman, "were observed between λ2000 and 1230."[1] Schumann, however, had succeeded in photographing some continuous maxima of which the most refrangible has a wave-length of about 1850 Ångstroms. Moreover, Lyman had observed that the great absorption band of oxygen diminishes in intensity as it approaches λ1230, but he thinks that another absorption band exists "lying in the region shut out by the absorption of fluorite." This preliminary investigation was undertaken, therefore, to test the emission and absorption of oxygen and air in the region of wave-lengths shorter than those transmitted by fluorite.

The apparatus used consisted of a vacuum grating spectrograph, containing a Rowland concave grating of 50 centimeters focus, about 15,000 lines per inch, and a ruled surface of approximately 2 inches. A discharge tube of internal capillary, end-on type and with aluminum electrodes was employed. The tube was also provided with a quartz window for Hg comparison spectrum and opened through a slit directly into the receiver. A method has been developed of making Schumann films, and these were used for the spectrograms. Commercial oxygen, dried with phosphorus pentoxide, filled the receiver and connected discharge tube to a pressure of about 0.4 mm. When the spectrum of air was obtained, this gas was likewise dried and filled the receiver to about the above pressure. The time of exposure varied from 20 minutes to 2 hours for the gas spectra, while an exposure of 3 minutes was found to be sufficient for the Hg-arc comparison spectrum. The apparatus was so arranged that both the first and second orders of the Lyman region

[1] Lyman, "The Spectroscopy of the Extreme Ultra-violet," p. 82.

appeared on the film, the second order being superimposed on the first order comparison spectrum. By the use of the foregoing method, an extensive spectrum was obtained with oxygen in the receiver and is attributed to that gas. A spectrum was also found for air. (See table.)

TABLE

1	I	2	I	3	I	4	I
990.2	1–8	1130.0	1	916.3	10	1136.4	1
91.7	1–8	68.5	1	1041.8	2	1249.4	1
1010.1	2	89.3	1	45.6	2	1216.0	15
36.9	5	1275.7	1	1188.4	2	17.5	1
84.3	4			96.0	2	1302.5	10
85.8	4			1224.3	1	5.2 t	9
1128.4	1			75.3 ^{2}	5	6.4	8
34.7	5			77.2	4	1713.9^{2}	2
76.2	10			1320.1	4d	30.8^{2}	3
80.7	1					43.7^{2}	1
84.8	1–10					75.0^{2}	2
1200.3	2					93.7	3
15.9	20					1812.3^{2}	3
61.6	2					61	3
1302.5	6					80^{3}	3
t							3
5.2	7					99.9^{2}	
6.4	6					1945.6d	3
11.2	1					50.4d	3
24.3	2						
30.0	1						
35.1	3						
36.2	3						

Explanation of Symbols of Table: } indicates doublet; } t triplet; } ^{1} doublet and probably triplet; d diffuse; ^{2} violet edge of band; ^{3} continuous maximum.

In almost every instance the wave-lengths given in the table are the averages of two or more plates, hence judging from the consistency of the several measurements they are believed to be accurate to about 0.5 Ångstrom. The first column contains the lines that occur in both air and oxygen. Column 2 gives the lines that were not registered on the films of air spectra, but were on those of the spectrum of oxygen. They are faint. Some of the more intense lines that occur in air only are indicated in column 3; the fainter lines, of which there are about thirty, were omitted from the list. Column 4 is a record of the wave lengths produced in oxygen with direct current discharge. The spectra listed in columns 1, 2 and 3 were obtained with disruptive discharge. In column 4, lines in the Schumann region are included; similar spectra were also present in the cases of disruptive discharge but were omitted from the tabulations.

The other columns are lists of the relative intensities of the wave-lengths in the columns immediately preceding. Where two values of intensity are given in the same column, the first refers to the spectrum with oxygen and the second with air.

It is worthy of note that the line λ1215.9 is very strong in the spectrum of oxygen and air even when a direct current was used. This wave length is very near to λ1215.6, the fundamental line in the hydrogen spectrum, and probably is that line. This was found to be present in most of the spectra obtained by Millikan in his investigations on the spectra of metals. The transparency of oxygen and air (1340–916 for air and 1336–990 for oxygen) in this region is proved from the fact that these spectrograms were obtained. It is evident that the absorbing layer of gas in these experiments amounted to more than 0.5 mm. at atmospheric pressure, and judging from the intensities of the spectra, these gases are transparent in layers of even much greater thickness. The films of the spectrum of air were badly fogged, and in some cases the entire spectrum appeared reversed. However, since other films of this spectrum were obtained without this reversal, it is believed to be of a chemical nature and due to the corrosive gases formed by the radiation or the discharge. This point will be investigated more thoroughly in the near future. The work of getting the spectra of electrolytic oxygen and pure nitrogen is now on the way, and the thorough search for series lines and for ionization and resonance potential relations is postponed until this new data is available.

J. J. HOPFIELD

DEPARTMENT OF PHYSICS,
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THE AMERICAN CHEMICAL SOCIETY.

(Continued)

DIVISION OF PHYSICAL AND INORGANIC CHEMISTRY

H. N. Holmes, chairman.

S. E. Sheppard, secretary.

Adsorption by precipitates V.: Adsorption during the precipitation of colloids by mixtures of electrolytes: HARRY B. WEISER. The precipitating action of mixtures of two electrolytes is approximately additive if the precipitation value of each is of the same order of magnitude such as frequently obtains when the precipitating ions have the same valence. The precipitating action of mixtures of electrolytes with widely varying precipitating power may be far from additive but under certain conditions may approach an additive relationship. The determining influences are (1) the effect of the presence of each precipitating ion on the adsorption of the other and (2) the magnitude of the stabilizing action of the ion having the same charge as the colloid.

The influence of the concentration of colloids on their precipitation by electrolytes: HARRY B. WEISER AND HENRY O. NICHOLAS. Burton and Bishop (*Jour. Phys. Chem.*, 24, 710 (1920)) state that the precipitating action of univalent ions increases and of trivalent ions decreases with decreasing concentration of colloid while that of divalent ions is almost independent of the colloid concentration. By an extended series of experiments with four different colloids this rule was shown to be far from general. With three of the colloids the precipitation value of all electrolytes decreased as the concentration of the colloid decreased, the effect being least marked with electrolytes having univalent precipitating ions. The determining factors are (1) the change in the amount of adsorption necessary for neutralization, (2) the change in the opportunity for collision of the particles, (3) the influence of the stabilizing ion particularly in the case of electrolytes that precipitate in high concentration.

Intermittent phosphorescence: HARRY B. WEISER. The luminescence of phosphorus is due to the rapid oxidation of phosphorus trioxide to phosphorus pentoxide. The luminescence is continuous only when the trioxide vapors are formed as rapidly as the luminescent reaction proceeds; the luminescence takes place in intermittent explosion waves when the velocity of formation of trioxide is less than the velocity of the explosion wave. The pulsations may be very rapid or may occur at intervals of several hours. The number of luminescent waves in unit time is determined by (1) the temperature, (2) the partial pressure of oxygen, (3) the extent to which the heat of reaction is absorbed by the containing vessel, (4) the presence of "catalytic" vapors.

The ternary system: silver perchlorate-benzene-water: ARTHUR E. HILL. Silver perchlorate is very soluble in water, and moderately soluble in benzene. The system has been studied from the temperature of the ternary entectic (—58°) up to the boiling points of the pure liquids. There occur four quintuple points, and twelve equilibria. Of most interest is the equilibrium in which three liquid phases are present, which may exist from —2.2° C. to + 22.3° C. It appears to be the only three-component system showing three liquid layers derived from components in which only one pair (water and benzene) show the formation of two liquid phases.

Hydrated oxalic acid as an analytical standard: ARTHUR E. HILL AND THOMAS M. SMITH. The common drawbacks to the use of hydrated oxalic acid as a standard for oxidimetry and alkalimetry are its retention of included water and its irregularity in combined water due to its distinct vapor tension. These two sources of error should be eliminated by fine grinding, to offer an escape for included water, and by drying to constant weight in an atmosphere in which the aqueous tension is exactly that of the hydrate. We have found that grinding to pass a 100-mesh sieve meets the first requirement. To meet the second, we have dried the compounds over a mixture of hydrated and dehydrated oxalic acid, which is the only drying agent which can be in equilibrium with the compound at all temperatures. The compound can be brought to a constant composition within about three hours, and agrees in its reducing action upon $KMnO_4$ with Bureau of Standards sodium *oxalate* within 0.03 per cent.

Effect of the history of adsorbent on adsorption: R. C. WILEY AND N. E. GORDON. Silica gel was prepared containing various amounts of water of hydration, and shaken with varying concentrations of different salt solutions. It was found that the amount of hydration had some effect on the adsorption of some salts. In most instances the change was very small, but in these cases the analytical method used made the

small changes as certain as where the change was more pronounced.

Adsorption from solution: D. C. Lichtenwalner, A. L. Flenner and N. E. Gordon. Varying concentrated solutions of calcium sulphate, calcium acid phosphate, magnesium sulphate, magnesium acid phosphate, potassium sulphate, and potassium acid phosphate were shaken with alumina hydrogel and iron hydrogel and the maximum adsorption determined by analyzing the solution before and after shaking. The water of hydration was all figured as water of dilution. Both gels show large adsorptions for each radical, and especially was this true in the case of the phosphate radical. The adsorption increased with increase of concentrate. The slow process of establishing equilibrium was also greatly marked.

Effect of hydrogen-ion concentration on adsorption: E. B. Starkey and N. E. Gordon. Hydrated gels of iron and silica were prepared in a very pure condition and shaken with a N/20 solution of KNO_3, K_2SO_4, $KHPO_4$. The hydrogen-ion concentration had been varied by the introduction of sodium hydroxide or hydrochloric acid as the case might allow. The adsorption of each ion was followed by analyzing the solution before and after the shaking and figure the water of hydration as water of dilution. It was found that the adsorption of the metallic ion decreased with an increase of hydrogen-ion concentration, while the nitrate, sulphate, and phosphate radical varied between no change of adsorption as in the case of the nitrate radical to a very noticeable change of adsorption in the case of the phosphate radical.

The sorption of toluene and acetic acid and their mixtures by carbon: A. M. Baker and J. W. McBain. A general method is described for determining the true sorption of both solvent and solute in place of the merely relative values obtained in the usual way for solutions. A maximum value for sorption is obtained which is independent of the absolute temperature; the ratio between the saturation values is that of the molecular weights (acetic acid being present as double molecules); and when solutions are employed, the total amount sorbed still corresponds to a complete monomolecular film in which a certain number of double molecules of acetic acid have replaced a corresponding number of molecules of toluene.

Drop weights of oils in solutions of emulsifying agents: Robert E. Wilson and Allen Abrams.

The preparation and properties of ferric hydroxide gel: Robert E. Wilson, William B. Ross and Leon W. Parsons.

The measurement of the plasticity of clays: Robert E. Wilson and F. P. Hall.

The transitional temperature of the sol and gel forms in gelatin: Robert Herman Bogue. Bingham has shown that viscous liquids can be distinguished from plastic solids by a measurement of the viscosity at varying pressures and an extending of the curves downward till they intersect the axes. The former type intersect at the apex of the viscosity-pressure axes, while the latter type intersect upon the viscosity axis. By applying the principle to gelatin solutions at different temperatures and employing the MacMichael viscosimeter at varying speeds of rotation in place of the capillary type at varying pressures, it is found that the gelatine follows the law for a viscous liquid at temperatures above 33 degrees C., while at lower temperatures it follows the law for a plastic solid.

On the swelling and gelation of gelatin: Robert Herman Bogue. Gelatine sols were treated with solutions of the silicates of sodium in which the $Na_2O : SiO_2$ ratio varied regularly from 1: 4 to 1: 1. The swelling, viscosity, alcohol number, and P_H values were determined. The data indicate that the effects resulting from such additions are due in all cases to changes in the P_H rather than to any other influence of the silicate. Gelation appears to be dependent upon the tendency of the substance to become solvated, the volume occupied by unit weight of dispersed phase being the determined factor. When this volume is very small or very large, the jelly consistency will be low, and at intermediate values of volume per unit weight the jelly consistency will reach its maximum.

Plasticity of colloids: Eugene C. Bingham.

The fluidity-pressure curves of gelatine solutions: S. E. Sheppard, Felix A. Elliott and Harry D. Gideouse. Gelatine solutions were studied whose concentration varied from 1 per cent. to 8 per cent. at temperatures of 25°, 28° and 30° C. The fluidities were measured with an Ostwald type viscometer under pressures up to 900 mm. water. Ordinary, de-ashed and a mixture of de-ashed and autoclaved de-ashed gelatines were used. All measurable solutions showed little evidence of plastic flow, the curves being linear and approximately intersecting at a common point.

The method of preparing the solution was shown to influence the slope of the curves.

The action of dilute chloride solutions upon silver chloride: GEO. SHANNON FORBES AND H. ISABELLE COLE.

The potentials at the junctions of chloride solution: D. A. MACINNES AND Y. L. YEH. E.m.f. measurements were made on cells of the type:

$$Ag/AgCl + MCl \text{ L } M'Cl = AgCl/Ag$$

(in which M and M' are the alkali metals and hydrogen) using a flowing junction similar to that developed by Lamb and Larson. With widely varying rates of flow the potentials were constant to $\pm$ 0.02 mv. for indefinite periods. With equal concentrations on both sides of the junction and assuming the chloride ion activity to be the same in all the solutions the measured e.m.f. is that of the liquid junction only. The results may be expressed by a simple additive relation in the few cases in which the formula of Lewis and Sargent does not hold.

Electrometric titration of ortho-phosphoric acid: E. T. OAKES AND HENRY M. SALISBURY. New curves for ortho-phosphoric acid titrated with sodium hydroxide and sodium carbonate are shown. These curves are plotted to show observed e.m.f. values as well as P_H values. Condenser method, and saturated calomel cell are used for measuring e.m.f. Technic of titrations, method of calculating results and sources of error are discussed briefly. Curves obtained by titrating phosphoric acid with sodium hydroxide, and sodium hydroxide with phosphoric acid are not mirror images. The second end point of phosphoric acid required more than twice as much alkali as the first. Curves obtained by titrating phosphoric acid with sodium carbonate, and sodium carbonate with phosphoric acid are vastly different. Equations conforming to these curves differ from those commonly accepted.

Oxidation-reduction potentials of certain indophenols and thiazine dyes: BARNETT COHEN AND W. MANSFIELD CLARK. A series of indophenols consisting of the condensation products of para-amino phenol with phenol, o-cresol, m-cresol, o-chlorophenol, guaiacol, thymol and carvacrol were synthesized. The potentials of mixtures of each of these with its reduction product were measured with a gold electrode at different P_H values. It is shown that the same general relations hold that were found by Clark in the study of methylene blue and indigo sulfonate, the potentials being a function of both the ratio of oxidation product to reduction product and of the hydrogen-ion concentration. The effect of substitutions in changing the characteristic potentials is noted. Previous work with methylene blue has been extended to other thiazines. Characteristic constants for thionine, gentianine, toluidine blue o, thiocarmine R, methylene green G and new methylene blue N have been established.

Oxidation-reduction potentials of sulfonated indigos: M. X. SULLIVAN AND W. MANSFIELD CLARK. A trisulfonate and tetrasulfonate were found to have identical characteristic potentials when each was in definite ratio to its respective reduction product. These potentials are distinctly more positive than those of mono- and disulfonates. The potentials of the mono- and disulfonates are approximately the same but more refined measurements will have to be made to distinguish them.

A series of oxidation-reduction indicators: W. MANSFIELD CLARK AND H. F. ZOLLER. It is shown that certain dyes are as susceptible to precise electrode study as are certain inorganic oxidation-reduction combinations. The great importance of hydrogen-ion concentration is emphasized. The potentials for each dye can be reduced to a characteristic value from which there may be calculated the hypothetical hydrogen pressures in equilibrium with the oxidation-reduction products. These values are used in the form log $(1/H_2)$ to which is given the symbol rH. Plotting the equilibria on the rH scale gives a picture of oxidation reduction indicators comparable with that of the acid base indicators plotted on the P_H scale. The following oxidation-reduction indicators were shown plotted on the rH scale: guaiacol indophenol, o-cresol indophenol, o-chloro indophenol, methylene green, thionine, methylene blue, indigo tetrasulfonate, new methylene blue, indigo disulfonate, neutral red and safranine. These constitute a series from rH 21.7, at the more oxidative end to rH 2.8 at the more reductive end of the scale.

Selenium galvanometric colorimeter: ALEXANDER LOWY AND OSWALD BLACKWOOD.

A submerged floating equilibrium bob that adjusts its weight to the density of the liquid in which it is placed: C. W. FOULK. This is a modification of the Richards floating equilibrium bob so that it can be used for the determination of the density of liquids over a considerable range. Preliminary experiments show that measurements of density can be made with it with an accuracy

of one or two in the fifth decimal place and probably in the sixth place, and that a given bob as modified will cover a range of about two decimal places, that is, with one instrument, for example, densities ranging from 1.00001 to 1.00010 could be read. The modification consists in attaching a light chain to the bob which is a fish-shaped, hollow glass, or silica bulb. It is evident that if the weight of such a bob (a certain amount of ballast is usually necessary) is approximately that of an equal volume of the liquid in which it is placed, it will assume a position of equilibrium between the surface of the liquid and the bottom of the containing vessel, the equilibrium being brought about by the chain suspended from its lower end. As the bob rises it lifts the chain link by link off the bottom of the vessel till the added weight counteracts the upward tendency and of course the reverse takes place if the bob tends to sink. A practical instrument utilizing this principle is made by having the bob in a tube open at both ends and with one end of the chain attached to the lower end of the tube, so that it hangs in a loop (catenary curve) between this point of support and the bob. The density of a liquid in which this instrument is placed can be determined by noting the position which the bob takes with respect to a scale on the tube. There are a number of interesting variations of the instrument that can not be given in a brief abstract.

The comparative value of different specimens of iodine for chemical measurements: C. W. FOULK AND SAMUEL MORRIS. Iodine was purified in various ways as described in the text-books of analytical chemistry and these preparations were then compared through the medium of a sodium thiosulphate solution with a specimen of iodine that had been purified as if for an atomic weight determination. Several new modifications of apparatus for purifying and drying iodine were also devised. The general conclusion drawn from the experiments was that the so-called "analytical" iodine is remarkably pure. Doubt, however, is thrown on the use of a sulphuric acid desiccator as a method of drying iodine when the water it contains had been entrained through the solidification of the iodine in the presence of liquid water.

Variation of grain size in photographic emulsions in relation to photochemical and photographic properties: E. P. WIGHTMAN, A. P. H. TRIVELLI AND S. E. SHEPPARD.

The physico-chemical properties of strong and weak flours III. Viscosity as a measure of hydration capacity and the relation of the hydrogen-ion concentration to imbibition in the different acids: ROSS AIKEN GORTNER AND PAUL FRANCIS SHARP. In continuation of the work reported at the Chicago meeting of the Society, the authors have applied the use of the viscosimeter to the study of hydration of the emulsoid colloids present in wheat flour. Instead of using the washed out gluten as in previous work a 20 per cent. suspension of the entire flour was used in the present study. The results indicate (1) that the viscosimeter affords an accurate and rapid means of measuring imbibition, (2) the form of the viscosity curves is identical with that of the imbibitional curves obtained previously by weighing gluten discs, (3) "strong" flours give greater viscosity values than do weak flours at the corresponding concentration of acid calculated on either normality or hydrogen-ion concentration basis, (4) when the viscosity is plotted against hydrogen-ion concentration instead of against normality of acid a radically different form of curve results, with a maximum viscosity at about $P_H = 3.00$, (5) the same value for maximum viscosity is not reached by all acids at the same hydrogen-ion concentration, (6) the order of the acids as influencing imbibition (lyotropic series) is not the same for all of the flours studied.

An interesting colloid gel: ROSS AIKEN GORTNER AND WALTER F. HOFFMAN. A rigid gel can be prepared from di benzoyl 1. cystine containing as little as 0.15 per cent. of the compound. Viewed by dark field illumination this is apparently a crystal gel. It is suggested that this material may assist in studies regarding gel structure for it can be easily prepared in pure cystalline form and is consequently not affected by previous history as is gelatin, agar, etc.

Are electrolytes completely ionized at infinite dilution? HAROLD A. FALES AND HAROLD E. ROBERTSON. Measurements made on hydrochloric, acetic, sulphuric and phosphoric acids up to a dilution of three million liters per mol, by the electromotive force method using the ballistic galvanometer, show that the thermodynamic ionization passes through a minimum and approaches zero with increasing dilution. It seems that it is not until a dilution of one thousand liters per mol is reached that the thermodynamic concentration of hydrogen ion becomes equal to the ionic concentration.

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